It is a deplorable fact that many cases of encephalitis lethargica are treated in a cavalier manner, dubbed influenza, and little or nothing done. This still goes on, although it is known that many of the cases with serious consequences do not seem bad at first; some do not see a doctor at all, and are only discovered later. But the first day may be the only time it is possible to detect the primary infection, so virulent, and so active in penetrating the cribriform plate of the ethmoid.

As some patients die in a few days no time is to be lost. The site of infection must be investigated thoroughly. Those of us who see the after-results in the form of dementia praecox, which would not have occurred otherwise, apparently inexplicable forms of manic-depressive mental disorder, and mental defect, which does not date from birth, know that even then swabs of the nose and throat nearly always show various pathogenic organisms. Even if these are only secondary infections they supply material valuable for treatment in the later stages, and indicate how much might have been effected at first by active local and general treatment for combating infection. But we have also noticed that many serious sequelae are often associated with other abnormal conditions, such as diseased teeth, and toxaemia of the alimentary tract. These must have been present at first, and may have determined the issue. But they are scarcely ever investigated, or at the most by a perfunctory question as to whether the bowels act every day. Further, it must be realized that the infected areas are at the base of the brain. Disorders of emotion are therefore much more common than any intellectual failure, especially at first. Usually this is not taken into consideration at all.—I am, etc.,

Birmingham, Dec. 18th, 1926. W. A. POTTS, M.D.

SANATORIUM TREATMENT.

SIR,—I have no claims to be considered an expert in statistics, but I have a certain knowledge of the clinical course of pulmonary tuberculosis in the district where I work, and I shall confine my remarks to the incipient cases considered in Dr. Stocks's memoir. I believe that the sanatorium-treated group was a selected group of patients with progressive disease for the following reasons.

(1) The fact that only 25 per cent. of incipient cases received sanatorium treatment suggests strongly that some method of selection was employed. The tuberculosis officers would urge treatment in those cases where it appeared to be most necessary: the patients would be more likely to accept their advice if they felt too ill to follow their normal mode of life.

(2) A study of the graphical representation of the progress of incipient cases on page 415 of Dr. Stocks's memoir supports this view. The sanatorium-treated group shows a higher percentage of "dead" and "progressive" cases at every single time period of the six years under review. This suggests to Dr. Stocks that sanatorium treatment had a bad effect: it suggests to me that this group contained a high proportion of patients with progressive disease.

(3) The patients were not reclassified at the time of their admission to the sanatorium.

(4) The data in the memoir support this view so far as they go. The difficulty is that these data are so inadequate as to be merely straws which show which way the wind is blowing. For example, the sanatorium-treated group shows a higher proportion of cases with marked systemic disturbance. This systemic disturbance was assessed apparently on a single observation of the temperature and pulse rate. If observations on the morning and evening temperature had been recorded for a fortnight before classification, they might well show a substantially higher proportion of cases with systemic disturbance in the sanatorium-treated group. If this was done there is no record of it in the memoir: if it was not done, the authors of the memoir are not in possession of the necessary data to attempt to assess the systemic disturbance. I submit that this factor alone might reverse the conclusions of the authors.

Again, the weights of the sanatorium-treated group fell on an average half a pound before admission, while the non-sanatorium-treated group increases by about half a pound during the same time interval. Dr. Stocks regards these differences as insignificant, but indeed he has already shown in the memoir that nutrition has only a small influence on progress (pp. 438-440). Yet in his letter of December 11th, 1926, he says that the existence of selection was precluded by the most accurate possible method—that is, a study of the weight curve. I concede his point that the differences of weight were insignificant in the different groups and advance further that the weighing machine is unable to show whether a patient is suffering from incipient "open" tuberculosis or incipient "closed" tuberculosis. This is shown by figures from the Forster Green Hospital. During 1921, 57 patients, classified as "incipient" on admission, were discharged. Of these, 24 had tubercle bacilli in the sputum, and their average weight on admission was 8 st. 11 lb.; their average gain of weight in the sanatorium was 8 lb. The other 33 patients, who had no sputum or in whose sputum no tubercle bacilli were found, had an average weight of 8 st. 10 lb. on admission and an average gain of weight of 9½ lb.

This leads one to inquire as to the proportions of T.B.+ and T.B.− cases in the sanatorium-treated and non-sanatorium-treated groups. But it has been pointed out already by several of your correspondents that this information is not available in anything like a complete form. Both in the memoir and in the ensuing correspondence Dr. Stocks has not faced the fact that the presence of tubercle bacilli in the sputum is the most important single factor in prognosis. Throughout the statistical tables two groups of cases with quite different prognosis are mixed in unknown proportions. As there are no complete records of sputum examinations, these groups cannot be disentangled by any statistician, however expert.—I am, etc.,

B. R. CLARKE, M.D.,
Medical Superintendent, Forster Green Hospital, Belfast.

December 14th, 1926.

CHOLECYSTOGRAPHY.

SIR,—In the issue of the BRITISH MEDICAL JOURNAL for November 6th (p. 864) I have noticed a letter from Mr. G. P. B. Huddy, M.S., F.R.C.S., on a fatality which occurred following the injection of sodium tetraiodophenolphthalein. So far as I know this is the only fatal case which might be considered due to the injection of tetraiodophenolphthalein. It has seemed to me advisable to call attention to the fact that not only was the dose nearly twice as much as the dose which my associates and I have recommended in our publications, but that also certain precautions which we have stated repeatedly as being desirable, if not necessary, were not taken in this case. Mr. Huddy states that the patient was given 5½ grams of sodium tetraiodophenolphthalein. The largest dose which we have given to a human being is 3½ grams, and we usually do not give more than 3 grams. We never give more than 0.06 gram per kilo of body weight. I cannot understand how Mr. Huddy could get the impression that 5½ grams was a suitable dose. In all of our publications we have advised strongly giving the dose in two injections about half an hour apart. By dividing the dose in this way a patient who happens to have an idiosyncrasy can almost always be detected before receiving the whole dose.

If the newer substance, which was recommended in my address before the British Medical Association, published in the BRITISH MEDICAL JOURNAL for October 16th, 1926, be used, a very much smaller dose can be given. We now use almost exclusively this new substance called phenoltetraiodophthalein, which is a chemical isomer of tetraiodophenolphthalein. A suitable dose of this newer substance for the average sized adult is 0.04 gram per kilo of body weight. We have never found it necessary, however, to give more than 2.5 grams of it to any adult. Since using this newer substance and exercising the precautions which we have always emphasized, we have had no serious toxic effects. This matter is taken up in detail in my article to which reference was made above. It should be mentioned also that only a pure drug should be used. Unfortunately at present it is impossible to exercise any kind of supervision or control over the manu-